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ever, the question is the somewhat simpler one of determining the conditions under which a bird can gain elevation without expending energy, velocities relative to the earth may, of course, be ignored.

There is, as I now see, a great advantage in making the simpler investigation first: for, as Dr. Kimball has clearly shown, as soon as we recognize the fact that the bird's motion relative to the medium depends only on their relative velocity, it becomes clear that gain of elevation, and consequently the whole phenomenon of soaring, is impossible in a uniform horizontal wind.

It follows that there was an error in my theory of soaring. Mr. Gilbert thinks it due in part to my assuming it to be possible for a bird to glide in a wind moving faster than itself, with its head to leeward; but I see no reason why birds should not accomplish this fact, and am satisfied that I have often seen them do it. He also holds that my bird, "in passing from a negative velocity relative to the air, to a positive velocity relative to the air, must pass through the phase of no velocity relative to the air, in which he is practically helpless." But I was dealing with the bird's component velocity in the line of the wind's motion; and he might always have a velocity relative to the air, though its component in that line might be zero. The error which I made was in assuming, that, under the conditions of flight to which I subjected my bird, the turn to leeward was possible. From the way in which I made him fly, it is clear that the resultant force exerted on him, at every point of his supposed path, must be upward and to leeward. That being the case, the turn to leeward could not be accomplished, and consequently the path he was supposed to describe was an impossible path.

I feel that I must apologize to those of your readers who may have followed me in what may fitly be called "a wild-geese chase."

J. G. MACGREGOR.

Dalhousie College, Halifax, N.S., March 8.

#### "Shall We Teach Geology?"

IN Professor Winchell's remarks on my review of his recent work, there are only two points that call for reply. First, as to the study of history, which, according to him, trains no faculty but verbal memory. He now says that his "criticisms on history contemplate it as a study urged upon children in the early stages of education," and that in the colleges it is pursued in a better way. But, even if imperfectly taught, history trains far more important faculties than verbal memory. It exercises the intellect generally quite as much as geology does, and it also calls into play the moral judgment and the sympathies, which geology does not. To Professor Winchell the old red sandstone may be a more important topic of study than the Roman Empire, and the plesiosaurus a more interesting object of contemplation than Washington or Columbus; but to the mass of men this is not so. As to the time that Professor Winchell would have spent on geology, I may have misapprehended his meaning; and, if so, I am glad to be corrected. I haven't his book by me at present; but, if I remember rightly, he says that the study ought to be taken up in the primary schools, and *continued through the various grades*, which I understood to mean that the subject should be studied more or less every year. He now says that he only wants it taken up several times at intervals, and not pursued continuously, which is more moderate. I do not see, however, how even so much study of geology is possible; because, not to speak of languages and literature, there are many sciences of greater importance than geology, which ought, therefore, to be studied first. Such are arithmetic and geometry, geography, physics, human physiology, psychology, ethics, civil polity, and history; and I do not see how even all of these can be taught in the public schools. If these views are correct, geology can be nothing but an optional study in the high schools and colleges, while in the lower schools it can have no proper place.

THE REVIEWER.

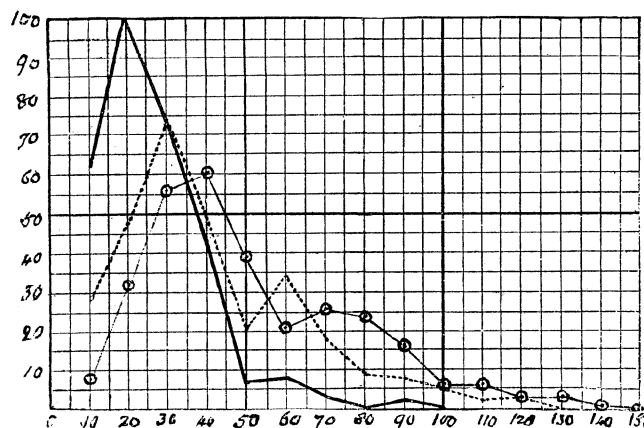
#### Curves of Literary Style.

IN the interesting researches on this subject by Professor Mendenhall described in your journal in 1887, words were classified according to the number of letters in them, and curves made ac-

cordingly. As he pointed out, there are many ways in which the principle of his method may be applied; and I have lately thought some instructive results might be obtained from examining sentences with regard to length, as measured by the number of words.

Length of sentences is a matter in which pronounced styles differ greatly. Doubtless this is associated with psychological peculiarities which it might be instructive to inquire into. The mental machine (so to speak) which, for example, turns out the long parenthetical sentences of Gladstone, must be very different in design from that which yields the simple and direct utterances of John Bright.

I have made an examination of 300 sentences in each of the following works: Carlyle's "French Revolution," De Quincey's "Confessions," and Johnson's "Rambler." The number of words in each sentence was counted, and the sentences grouped accordingly. Then the sentences with words up to 10 were added together, those with words from 10 to 20, from 20 to 30, and so on. The accompanying curves were then obtained from these data. Let it be clearly understood what they mean. The plain line curve (for Carlyle) means that in the 300 sentences of the passage selected there were 62 containing words varying in number up to 10, while 100 had from 10 to 20, and so on. The result is roughly as we might expect: short sentences form the bulk of the Carlyle passage, his maximum being in the class 10 to 20, and sentences of more than 50 words are comparatively few. There are none beyond 100. De Quincey and Johnson, on the other



ANALYSES OF STYLE FROM CARLYLE, DE QUINCEY, AND JOHNSON.

Carlyle, heavy line; De Quincey, broken line; Johnson, light line with dots.

hand, have an abundance of longer sentences. De Quincey's most numerous class is that of 20 to 30 words; Johnson's, 30 to 40. But the curve of the former does not die down till after 110 to 120 words (really there was one inordinate sentence of 170, not shown in the diagram); while Johnson's is further protracted to 130 to 140.

I do not affirm the constancy of these curves: they only apply to the specified passages of 300 sentences. These few lines are merely by way of suggestion, and should any reader have the time and patience to pursue the inquiry further, he might, I think, find his labors not without some useful results.

It might be useful to see in what degree these curves approximate to constancy, or come short of it. One would like to know better than we do at present, how far the method, in any of its forms, is reliable or helpful in settling disputed questions of authorship, or in tracing anonymous literature to its source.

I would suggest an examination of the words used by speakers or writers as likely to be instructive.

A. B. M.

London, March 7.

#### Wind-Velocity and Wind-Pressure.

FROM time to time there have appeared discussions of these questions, so important to the practical engineer. It seems probable that the first of these, as far as relates to the relation between wind-movement and the travel of the cups of Robinson's anemometer, is soon to be definitely settled by indubitable experiments.